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VOLUME XXXV No. 661 NOVEMBER-DECEMBER 1950

E.S.P. PERFORMANCE AND THE RORSCHACH TEST

A SURVEY OF RECENT EXPERIMENTS

BY GERTRUDE R. SCHMEIDLER

I. THE RATIONALE OF A SYSTEMATIC STUDY OF THE PERCIPIENT

To those who are interested in psychical research it often seems that inexplicable as its occurrences may be, some of them follow a recognizable pattern. Some gifted persons, for example, have more than their share of psychic experiences. Perhaps these experiences occur more readily to certain *kinds* of individuals than to other kinds. If this is the case, it would be well worth while to investigate the characteristics of the more sensitive as opposed to the less sensitive type of person. Fruitful results from such a programme could give us a firmer basis for speculation as to the nature of psychic events, since they would show some of the operative conditions. They would teach us how to recognize the gifted person who was not yet aware of his special ability. A third possible advantage, which may seem fantastic at the present stage of our knowledge but is not impossible *a priori*, is that any characteristic which differentiates the more psychic from the less psychic individual may be susceptible to artificial control. Such control would be an indirect way of developing or inhibiting psychic abilities.

My first point, then is that anyone who considers it reasonable to look for a gifted sensitive, or to work with a sensitive on the basis of that individual's past successes, is assuming implicitly or explicitly that some quality differentiates the sensitive from other persons. He should therefore consider it profitable to search directly for this quality.

There are other types of psychic events which seem to be predictable. One observation that has often been repeated is that good *rappport* between percipient and experimenter is conducive to better results than a cold, suspicious, or unfriendly atmosphere.

This statement has been made both in reference to gifted sensitives and to unselected subjects (see, e.g., 2, 3, 9). With unselected subjects, also, an early investigation by the S.P.R. indicated that psychic phenomena occurred more often in connection with death than would have been expected on a chance basis (5). Can we generalize, as Murphy does, that psychic events tend to occur 'in such fashion as to reduce tension, or to meet needs' (8) as do so many other psychological phenomena? If we consider it likely that an experiment will give better results when agent and percipient are friendly than when they are hostile, or that psychic perceptions cluster around the time of a crisis in the life of someone we love, we are assuming that a person's emotion or attitude or mood is one of the determinants of the occurrence of psychic events. Granted this assumption, it becomes reasonable to study mood, or attitude, or a person's way of looking at the world concomitantly with a study of *psi*, so that we can better develop a technique for predicting or controlling the occurrence of psychic phenomena.

2. TECHNIQUES FOR STUDYING THE INDIVIDUAL

How are we to make these studies? Several approaches are possible, and they may be combined or used independently of each other. Perhaps the most obvious is the biographical. Here the investigator would try to determine if common experiences, perhaps in early childhood, are shared by many sensitives but occur infrequently in the lives of others. Another line of research would be the physiological, where the trained investigator would examine such factors as autonomic function, or endocrine balance, or the pattern of the electro-encephalogram, to see if he can distinguish sensitives from others, or if he can distinguish the periods when successful responses are made in the course of psychic research. A third possibility, psychological investigations, will be described in more detail.

(a) *Introspection*

Probably the simplest of all psychological methods, with human subjects, is to ask the subject what he thought or felt. If the answers are taken at their face value, they are likely to be so variable and inconclusive as to discourage the research worker. The reasons are manifold. Two are semantic: we all tend to build up our personal associations for words, and thus use them to connote somewhat different shades of meaning. We also, when we speak non-technically, are likely to use terms which are too general, and which thus obscure important points of difference.

A third reason is that we are often not aware of certain unconscious determinants of our behaviour, and thus cannot report on them in our introspections. A fourth is that the process of introspection may alter the process that is to be observed: a subject who knows he will be asked how or why he reacted may be so self-conscious that he reacts in a different way. In the light of these difficulties, most experimenters would be well advised if they made use of the introspective method only as an auxiliary technique. For most purposes, it is sufficient to ask a single non-leading question, to record the subject's spontaneous response, and not to probe for more.

(b) Observations of Behaviour

Another method, equally obvious and often very useful, is to record as much as possible of the subject's behaviour. His tension or relaxation, the times when he yawns or glances around the room or taps his fingers on the table, his suggestions that the procedure be modified, the tempo of his movements—all these and many more can give useful clues as to variations of his mood which are so fleeting, or so apparently causeless, that he himself might not be able to describe them. It happens rather frequently that after an experiment designed to test a hypothesis has given inconclusive results, the experimenter is able to infer from such observations what had gone wrong the first time. He can then re-design his procedure, and may succeed in making a clearcut test of his reformulated hypothesis on the second try.

(c) Experiments

A third familiar method of obtaining psychological data is the experimental. Certain conditions are varied systematically; all other conditions are held constant, in so far as the experimenter can do so; and data obtained under these different conditions are compared. In theory the method is simple and straightforward; but in practice it is extremely difficult to ensure that all relevant conditions other than the independent variable have in fact been held constant. Let us suppose, for example, that someone were to perform an experiment to test the theory that was cited earlier, that a percipient will be more successful in a friendly atmosphere than in a cold, formal, unappreciative one. The experimenter has already, let us say, found a sensitive who has obtained high scores in several previous experiments, and two associates, one of whom is responsible for creating the friendly, and the other for creating the formal atmosphere. When he has collected half the data, he finds that the results are in line with the predictions. At this point the friendly colleague catches pneumonia. Must the experi-

ment stop? Only a little thought shows that it should continue ; it was designed to test the percipient's response to anyone who was friendly, rather than to a particular individual. Another equally agreeable colleague substitutes for the first, but after a few sessions the scoring level drops to chance.

Where the original hypothesis can no longer explain the data the experimenter may turn to one of the other psychological methods. From the percipient's behaviour and comments it may become clear that he reacts differently to a man than to a woman, or perhaps to a contemporary, as contrasted with someone very much older or younger, even though they are equally friendly. Or perhaps the percipient was beset by personal worries during the latter weeks of the project, and what seemed a response to a minor change in the conditions of the experiment was in fact a reaction to conditions which the experimenter had not taken into account. Or—to list only one other possibility, among hundreds which might be suggested—at the beginning of the experiment the percipient had felt uneasy and unsure of himself, and it was only when reassured by warmth and encouragement that he could succeed. As the experiment continued and his confidence increased, his interest waned, so that the very reassurance which had helped him at first was discouraging later, and only a challenging, sceptical experimenter could re-establish an attitude like his earlier one. Thus a revision of the first simple theory might be in order, in terms of the particular needs, or personality pattern, of the sensitive who is being studied. When the revised hypothesis has been stated, it can, in its turn, be investigated by the experimental method.

The question might be raised as to whether the study of one individual can ever be sufficiently objective and verifiable to deserve to be used in scientific research. One affirmative answer is based on the use of psychological tests, which are attempts at systematised methods of studying individual differences. They are usually divided into two major categories : non-projective and projective tests.

(d) Non-projective Tests of Personality

Some tests have been so constructed as to be easy to administer and to score. Almost all of them take the form of a list of questions about the subject, which he must answer by checking one of various possible responses on the paper before him. For example, to the statement, 'People often come to me for advice,' he must check either 'Yes' or 'No' ; or to the question, 'Do you take the lead in enlivening a dull party?' he must check either 'Often' or 'Seldom' or 'Never'. Where the questions have been care-

fully selected and clearly phrased, and the responses have been standardized on an appropriate population, scores often reveal personality traits. In one of the best tests of this type, for example, the Allport-Vernon Study of Values (1), the scores show the relative importance of theoretical, economic, aesthetic, social, political, and religious values. If this test were administered to subjects in a *psi* experiment, the experimenter might find that subjects to whom political (power) values were extremely important would make good agents in a telepathy experiment but be ineffective as percipients; or he might find that subjects with strong social values would succeed in a telepathy experiment where the experimenter showed keen interest in their success, but give only chance scores in a clairvoyance experiment where the targets were conventionalised symbols and the experimenter's attitude was impersonal. In both cases, subjects whose values were predominantly theoretical, or predominantly economic, could be expected to score at approximately the same level even under such markedly contrasting conditions.

Tests of the question-and-answer type have a limited usefulness. They depend on the subject's frankness, and upon his accurate evaluation of himself. Another limitation is that data from them should be interpreted only for the trait for which the test was designed. They give a series of quantitative scores on a predetermined continuum; they cannot represent the subtle and complex picture of the personality as a whole.

(e) *Projective Tests of Personality*

The 'projective' tests (4) meet both of these difficulties. They present to the subject an ambiguous stimulus, and ask him to work with it. In one of the most widely used of these tests, for example, a series of twenty pictures is shown, of which the first represents a boy bending over a violin which lies on a table before him (6). The subject is asked to tell an imaginative story based on this scene. It is found that the subject will, in almost all cases, 'project' some of his own wishes or feelings or fears on to the chief character. To give only three examples, an adolescent who resents parental restrictions but is outwardly compliant is likely to say, 'This boy obviously (*sic*) has been told by his mother that he must practise the violin. He did some exercises and now he is resting, and wishing that he could go out to play.' A confident subject who has high ambitions and is willing to work hard to achieve them is more likely to tell some such story as the following: 'This is a musical genius. He has been working over a difficult composition, and now is thinking about how he should play the next passage. In a moment he will take up the violin and play his

own interpretation of it. At his recital, the critics will acclaim this interpretation and say that he is a great violinist.' A story told me by a timid and awkward seventeen-year-old ran something as follows: 'This boy's father is a musician. His Stradivarius is lying on the table, and the little boy is looking at it. You can see from the expression on his face that he is thinking of the beautiful music that the violin can make. He picks it up, to try to play it like his father, when he hears the door of the room opening. He jumps up, and the violin drops from his hands and breaks.'

In a test of this sort, the subjects' responses are infinitely varied. Each person has an opportunity to express his unique personality patterns, and even to express attitudes of which he himself is unaware. An increasingly large number of psychologists feel that, if they are to gain insight into the individual's patterns of behaviour, tests of this type are the only substitute for a very long series of intensive interviews. It is the purpose of this report to describe research performed with one such test, the Rorschach, and to suggest further research which might profitably be done along the same lines.

3. THE RORSCHACH TEST

The Rorschach is the most widely used, and probably the most useful projective test. Invented by a psychiatrist (10), it has proved itself able to distinguish the basic psychiatric categories of the neuroses and psychoses, and even, sometimes, the presence and location of brain tumours. More important for our purposes, it often shows with remarkable subtlety the 'structure of the personality': whether the subject is characteristically rigid and formal in his approach to situations; whether he shows an initial shyness which is readily overcome; if he is quickly impulsive, and later regrets it; if he is vigorous, or creative, or meticulous, or sensitive, or anxious; if he responds to social warmth; if he drives himself too hard intellectually; if he surrenders quickly under difficulty; and so on *ad infinitum*. Since the test will usually take only an hour to administer (though considerably longer to interpret) it is obviously a useful instrument.

The test is a simple one. It consists of a standard series of ten inkblots. These are shown to the subject in a predetermined order, with instructions for him to tell what he sees in the blots, or what they might be. Later inquiry seeks to determine how and where he saw his various responses, and (if his record was too limited), whether he would be able to see a wider variety under encouragement. It thus represents a typical projective test, since the stimulus and instructions are deliberately ambiguous, and the

subject must, if he is to see anything at all in an unstructured inkblot, metaphorically *project something of himself into the material*.

Interpretation of the record involves scoring the responses according to some half-dozen categories, the most important of which are reaction time, location (whether the response refers to the whole blot, to a commonly used detail, to an unusual detail, and so on), 'determinants' (whether the response was determined by the shape, colour, or shading of the blot, etc.), content and originality. Interpretation depends on the absolute number of responses in each category, the percentage of each in relation to the total number of responses, variations among responses given to the different cards, and of course the characteristics of the individual responses themselves. A full interpretation is so time-consuming that the research worker must choose between working in detail with a small number of records or making a more perfunctory study of a large number. The report that follows will discuss the latter approach.

4. BACKGROUND OF THE EXPERIMENT

The background of this research lies in experiments performed by the author during the years 1943 to 1945 (12, 13). In six series of individual experiments and one series of classroom experiments, subjects were divided into two categories, the 'sheep' and the 'goats'. Sheep were defined as those who accepted the possibility of paranormal experience under the conditions of the experiment; the goats were those who rejected this possibility. The data, summarized in Table 1, show that higher average E.S.P. scores were obtained by sheep than by goats. This was interpreted to mean that subjects with a friendly or interested attitude towards the experiment tended to score above chance expectation, whereas subjects with a negative, disapproving, or hostile attitude tended to react negativistically and to score lower.

If this generalization is valid, it raises the question: why is it not more readily demonstrated? Even in these early experiments, a substantial minority of the subjects scored in the direction opposite to the predicted one. Is this due to the fact that many subjects have no *psi* ability, and therefore their scores show chance variation? Or is it because other factors are present, which have a stronger effect upon the results than the intellectualistic attitude of theoretical acceptance or rejection which is being examined? The preceding section of the report indicates that one way to test this possibility is to administer personality tests to the sheep and the goats, in order to find whether some other factors differentiate the high scorers from the low.

Preliminary work with the Rorschachs of the eighty-five subjects of two of the later series included in Table I indicated that the general social adjustment of the subjects might be relevant. An objective technique for determining this factor from Rorschach records was described by Munroe (7). She administered Rorschachs to college freshmen, and determined from each record a figure which served as index to social adjustment. College grades could be predicted from this index about as accurately as from the results of intelligence tests, since the subjects with the best adjustment tended to do the most successful work, and the students with the poorest adjustment were most likely to fail in their courses. (Intelligence tests and adjustment scores in combination gave, of course, a much more accurate prediction than did either alone.) These results indicate that well-adjusted students could make productive use of their intellectual abilities, but that the students with many personal difficulties were likely to be too concerned with their own problems to react in the expected, adaptive way in a college setting. The same principle seemed to be at work in our preliminary experiments with E.S.P. An extensive series of experiments was accordingly undertaken to find whether the sheep whose Rorschachs showed them to be well adjusted would have *higher* E.S.P. scores than the other sheep, and whether the goats whose Rorschachs showed them to be well-adjusted would have *lower* E.S.P. scores than the other goats.

5. PROCEDURE

In the series of experiments¹ which I have been asked to review here (14, 15, 16) all subjects were college students, and all tests took place during class periods. The only basis for selection of subjects was the willingness of the instructor to permit the class to be used for research. There were twenty-seven separate administrations, and a total of 579 subjects. One experiment was administered by Professor Gardner Murphy, one by Professor Bohdan Zawadski (both of City College), and the remainder by the author. Sixteen of the classes were taught by the author; nine were borrowed from other instructors.

Although we attempted to keep the procedure uniform in all essentials, there were inevitable minor differences from one session to the next. It would be tedious to list them, and would serve no useful purpose. I propose instead to describe the points

¹ One series was performed as Research Officer of the American Society for Psychical Research; all of the research was financed (or partially financed) by the Richard Hodgson Fund of Harvard University. Professor Gardner Murphy, as Hodgson Fellow and as Chairman of the Research Committee of the A.S.P.R., has directed the entire project.

which were the same in all classes, and then to give a more detailed account of one typical session.

The targets at which the subjects were asked to guess were lists of the five E.S.P. symbols developed at Duke University: circle, square, star, cross, and wavy lines. In all cases the target list was prepared in advance by an assistant who was not present at the experiment. The order of the symbols was not known to the experimenter. 'Open' lists were used, where each symbol could appear a different number of times in successive lists. The order of items was determined from a table of random numbers. Lists were concealed from both subjects and experimenter until all guesses had been completed. The symbols were arranged in lists of twenty-five units, each of which was called a 'run'. All subjects were instructed to make nine runs, writing their guesses on prepared record blanks. All records were given two or more independent scorings. Each subject was required, before making his first guess, to state whether he accepted the possibility of paranormal experience under these conditions (in which case he was to call himself a 'sheep') or whether he rejected all possibility of paranormal experience under these conditions (in which case he was to call himself a 'goat'). In another session, all subjects took the Rorschach test. All Rorschachs were scored by the experimenter without knowledge of the subject's E.S.P. score.

In a typical session, each student received a set of papers as he came into the classroom. One was a half-sheet on which was mimeographed a space for the subject's name and the words, 'I am a sheep/goat because . . .' These name sheets were numbered serially, so that a distinguishing number was assigned to each subject. The other papers consisted of two identical pages, stapled with a sheet of carbon paper between them. On each was mimeographed nine columns of twenty-five boxes. Double lines were drawn under every fifth box. The upper sheet and the carbon paper were cut vertically between the third and fourth columns, and also between the sixth and seventh columns. The number which was on the name sheet was written above the second, fifth, and eighth columns.

When the class period began, the general problem of the research was described briefly, and the procedure described in detail. The students were then asked to classify themselves as sheep or goats, and to guess at the first three runs, that is, to record seventy-five E.S.P. responses. After about five minutes, those students who had not completed the three runs were urged to hurry; and all students were required to tear off the strip of paper as soon as three runs were recorded on it, and to pass it to the experimenter. When all strips had been collected the opaque

envelope containing the targets was opened, and the experimenter read the lists aloud. The subjects consulted the carbon copies of their responses to check the symbols which coincided with the targets.

The experimenter next distributed Stuart's Interest Inventory (20) to the class. They completed it, and then were asked to guess at the next three runs. Again the strips on which they had written were collected, the target envelope opened, and the lists read to them. Copies of Rosenzweig's Picture-Frustration Study (11) were distributed and the subjects filled in the blanks. They then guessed at the last three lists, and if time permitted before the close of the fifty-minute period, these strips were also collected and the last set of targets read to the class.

In another session the Rorschach Test was administered to the class, with instructions patterned after those of Sender (18). E.S.P. subjects who were absent for the Rorschach were encouraged to take the test individually. Whenever possible, group Rorschach records were supplemented by individual inquiry.

6. DISCUSSION OF THE PROCEDURE

This method of experimentation deserves to be criticised on two counts. In the first place, the group administration of the Rorschach loses much of the richness of the individual test. This is, however, a relatively unimportant point, since the group test retains sufficient sensitivity to make it a valid instrument (7).

More important are the defects of the E.S.P. test. In a procedure which is so cold, hurried, and mechanical, all the prescriptions for successful *psi* experiments would seem to be disregarded. In partial extenuation, we may plead that conditions were so carefully controlled that it would be hard to imagine any sensory cues which could have helped the subjects make correct responses. But this is not sufficient reason for some of the unattractive aspects of the method. It would, for example, have been possible in many cases to have an agent in another room looking at the symbols; for many of the subjects this would have made the material seem closer and more interesting. It would certainly have been possible to require fewer guesses; and this would probably have reduced both boredom and need for speed, and thus probably have raised the scoring level. The critical reader may well feel that if *psi* can be shown to operate with unselected subjects under the routine used in this research, then its field of operation is probably very wide. He may also feel that if a corresponding series of experiments was undertaken with more sympathetic attention to the interest of the subjects, it would be more successful than the research being described.

7. RESULTS

These experiments (14, 15, 16) show the same trend as those made prior to June 1945, but the trend is much less pronounced (Table 2). The sheep, with 3,000 runs, had a deviation of +287 and a mean of only 5.10. The goats, with 2,205 runs, had a deviation of -119. The difference between the mean score of the sheep and of the goats was .15 in favour of the sheep; in previous work it had been .38.

When we consider only the subjects whose social adjustment was good, we find (Table 3) that the contrast between sheep and goats is more striking (14, 15, 16). The mean E.S.P. score of the 209 well-adjusted sheep was 5.18; for 150 well-adjusted goats it was 4.84. The difference between the means is statistically significant ($P = .000001$). This confirms the observations made on the preliminary group of 85 Rorschach subjects, described in the section entitled 'Background of the Experiment'. (The Rorschach data from this group are not included here.)

After 250 subjects had been tested in these experiments, an analysis was made of their Rorschach scores. It was found that seven factors (or, in Rorschach terminology, 'signs') appeared much more frequently in the records of subjects with poor E.S.P. scores than in the records of subjects with good¹ scores (14). The records of 329 later subjects were examined (Table 4) and showed a similar trend (15, 16). For this latter group, the difference between E.S.P. scores of sheep and goats, for those subjects whose Rorschach protocols were free of the seven signs, was .46 in favour of the sheep. This is significant at the level of $P = .0003$. Table 5 analyses the seven signs, for this group of records. The signs are defined as follows:

R + : more than 30 responses in a group test; more than 50 responses in an individual test.

F% + : 50 per cent or more of the responses determined primarily by form.

MR : more than half of the human movement responses characterized by rigidity.

TOTAL MOVEMENT + + : 60 per cent or more of all responses showing movement or expression.

NO SHOCK : neither colour nor shading shock, even when both are scored very lightly.

CF + : 2 or 3 strong or whole colour-form responses if there

¹ 'Good' E.S.P. scores are taken to mean scores above chance for the sheep or scores at or below chance for the goats. All other scores are called 'poor'.

is no more than one main form-colour response ; or at least twice as many colour-form responses as form-colour responses.

C + : 2 or more colour description or colour symbolism responses ; or one or more pure colour responses.

8. DISCUSSION OF THE RESULTS

One conclusion which may be drawn from our data is that in these experiments there is a tendency for the attitudes of the subjects to influence their scoring levels. The subjects who rejected any possibility of paranormal success under the conditions of the experiment, and thus presumably had an unfavourable attitude towards the experiment, had lower average scores than those who accepted this possibility.

Consideration of the data shows that this is, however, only a partial tendency. There was a great deal of overlap between our two groups of subjects. Thus our second conclusion must be that, even though this factor is operative, there are other factors which can counterbalance it.

We shall suggest two possible factors. One is that intellectualistic attitudes of acceptance or rejection are not important to some individuals. A subject who likes games may enjoy making competitive guesses, even though he does not believe he can influence the results. He may thus have a favourable attitude towards the experiment, though he has been classed as rejecting it on intellectual grounds. Another subject, torn by conflicting feelings, might fear that psychic events occur but hope that they do not. Classed in the accepting group, he may still have a negative attitude towards paranormal success. Perhaps our sheep-goat criterion is differentiating in respect to E.S.P. score only for the subjects with high theoretical values (i.e. positive response to theory) on the Allport-Vernon Scale!

Another possibility is that the way the experimenter handles the situation is a crucial determinant. In my own earlier research, for example, there was still some residue of scepticism in my own mind as to the occurrence of these phenomena. The goats may have recognized this, and thought that it would be possible to convince me by low scores that paranormal success was impossible. No such challenge would be felt by my current subjects. Again, the disapproval of the procedure which is experienced by many of the goats is given an outlet by the fact that they describe their attitude before they make their guesses. Perhaps, if no such outlet were provided until later, they would feel the more thwarted by the procedure, and show more negativism in avoiding the targets. This possibility is, of course, readily amenable to test.

Our third conclusion is that subjects who are well adjusted give less equivocal data than subjects whose adjustment is poor. This is reminiscent of the statement of Smith and Humphrey (19) that since secure subjects made higher E.S.P. scores than insecure subjects, 'the E.S.P. function, if not a sign of "normality" itself, at least operates best in well-adjusted individuals when a random selection of the population is tested.'

One possible reason why randomly selected, well-adjusted subjects score better than randomly selected subjects whose social adjustment is poor is that the experimental situation is not a very compelling one. For most subjects, interest in the experiment and desire to succeed represent only a general interest in any project, rather than deep concern with paranormal ability. Well-adjusted subjects are, all other things being equal, more likely to feel interest in any situation, and to succeed in it within the limits of their ability, than are poorly adjusted subjects. Members of the latter group are more likely to be influenced by factors which are not apparent to the experimenter: they may show an intense desire to succeed, or an excessive hostility, or may be so withdrawn that they disregard the demands of the objective situation. They are less predictable as a *group*, although of course a careful study of individuals makes it possible to offer individual predictions. To turn for analogy to the relation between adjustment and college success: well-adjusted intelligent students are almost sure to succeed in college. But within the poorly adjusted intelligent group one student may throw himself so heartily into his studies that he will make an extraordinarily brilliant record; another may be so concerned with other problems that he will fail; and the group *as a whole* is less predictable than the well-adjusted one.

Analysis of the individual Rorschach categories of Table 5 must be extremely tentative, since the data are not statistically significant. We can wonder if the last, and perhaps the first signs should be omitted from our list; we might infer that the remaining five, under the conditions of the experiment, contraindicate E.S.P. success (as here defined). It is, in a sense, misusing the Rorschach to consider a single score out of context, but to the extent that it is permissible to do so, these data and those of Table 4 seem to point to a fourth conclusion: that, under the conditions of the experiment, a cold withdrawn attitude ($F\%+$, Mr, No Shock) militates against good E.S.P. scores, as does marked impulsiveness or lack of emotional control ($CF+$) or excessive, almost compulsive mental activity ($R+$, Total Movement $++$). We can speculate as to whether these tendencies would operate in the same way in a different situation, but have no right to assume that they would do so (15).

9. SUGGESTIONS FOR FURTHER RESEARCH

These experiments raise more questions than they answer. Perhaps the most basic problem is this: Is *psi* ability so widespread that proper administration of an experiment, with detailed study of individual attitudes, can show that each subject demonstrates these powers? The assumption that underlies the research just reported is that the answer to this question is an affirmative; and our data support, though they do not establish, this affirmative answer. The crucial research would deal with a large number of randomly selected subjects; would study each with great care; would predict, for each, conditions under which he would score above chance, or conditions under which he would score below chance, or both; and would test him under these conditions.

Another question deals with the characteristics that were related to poor scores. Do subjects with the Rorschach signs listed above, or subjects with poor general adjustment, typically show less *psi* ability than other subjects? Probably they do not; internal evidence indicates that different conditions would show that different, but equally consistent, factors were the crucial ones for some other experiment (15). If this is true, we have not answered the question with which the research began: Are there characteristics which reliably indicate the presence (or absence) of psychic ability? Perhaps an objective study of the experimenter's relation to the subjects, combined with more subtle use of psychological methods like the Rorschach, can provide the answer, even if this research does not.

There is a great deal of material concerning these 579 cases which has not yet been properly examined. The interrelations among Rorschach categories have hardly been studied, for example; and yet these interrelations are of more interest than the single signs that were emphasized here. Other tests used with the subjects have been only partially reported (17). This gives rise to the further questions: Might other measures be more revealing than the ones we have used? Or might not the comparison of data from several approaches give us more important information than the use of only one or two? In a field where there has been so little systematic research, and where the work that has been done offers such interesting leads, the need for further investigation seems great.

*Department of Psychology,
The City College,
New York, N. Y.*

TABLES

TABLE 1

E.S.P. SCORES OF SUBJECTS TESTED BEFORE RESEARCH BEGAN WITH THE RORSCHACH TEST 1943—JUNE 1945

		Number of Subjects				Runs	Dev.	Mean
		Scores above chance	Scores at chance	Scores below chance	Total			
Sheep	-	80	9	43	132	1248	+278	5.22
Goats	-	25	2	45	72	1143	-187	4.84

TABLE 2

E.S.P. SCORES OF RORSCHACH SUBJECTS IN CLAIRVOYANCE-TYPE TESTS
OCTOBER 1945—DECEMBER 1948

		Number of Subjects				Runs	Dev.	Mean
		Scores above chance	Scores at chance	Scores below chance	Total			
Sheep	-	172	18	144	334	3000	+287	5.10
Goats	-	109	6	130	245	2205	-119	4.95

TABLE 3

E.S.P. SCORES OF SUBJECTS WHOSE RORSCHACHS SHOWED THEM TO BE WELL ADJUSTED AND OF SUBJECTS WHOSE RORSCHACHS SHOWED THEM TO BE POORLY ADJUSTED

WELL-ADJUSTED SUBJECTS								POORLY-ADJUSTED SUBJECTS							
Number of Subjects				Runs	Dev.	Mean		Number of Subjects				Runs	Dev.	Mean	
Scores above chance	Scores at chance	Scores below chance	Total					Scores above chance	Scores at chance	Scores below chance	Total				
124	11	74	209	1879	+324	5.17		48	7	70	125	1121	-37	4.97	
59	4	87	150	1349	-206	4.85		50	2	43	95	856	+87	5.10	

TABLE 4

E.S.P. SCORES OF SUBJECTS WHOSE RORSCHACHS WERE FREE OF SEVEN 'SIGNS' *
AND OF SUBJECTS WHOSE RORSCHACHS SHOWED ONE OR MORE OF THE SEVEN 'SIGNS'

SUBJECTS WHOSE RORSCHACHS WERE FREE OF THE SEVEN 'SIGNS'								SUBJECTS WHOSE RORSCHACHS SHOWED ONE OR MORE OF THE SEVEN 'SIGNS'							
Number of Subjects				Runs	Dev.	Mean		Number of Subjects				Runs	Dev.	Mean	
Scores above chance	Scores at chance	Scores below chance	Total					Scores above chance	Scores at chance	Scores below chance	Total				
45	3	19	67	603	+147	5.24		67	11	72	150	1348	+29	5.02	
12	2	26	40	360	-78	4.78		40	1	31	72	648	+86	5.13	

* See page 333.

TABLE 5

E.S.P. SCORES OF SUBJECTS WHOSE RORSCHACHS SHOWED ONLY ONE OF THE SEVEN 'SIGNS'

	Number of Subjects				Runs	Dev.	Mean
	Scores above chance	Scores at chance	Scores below chance	Total			
Sheep whose Rorschachs showed :							
R+ - - - - -	13	2	9	24	215	+32	5.15
F%+ - - - - -	8	2	10	20	180	-22	4.88
Mr - - - - -	6	2	7	15	134	-21	4.84
Total Movement++ - - -	9	2	13	24	216	-19	4.91
No Shock - - - - -	1	0	5	6	54	-17	4.69
CF+ - - - - -	2	0	8	10	90	-13	4.86
C+ - - - - -	1	0	0	1	9	+3	5.33
Goats whose Rorschachs showed :							
R+ - - - - -	5	0	3	8	72	+15	5.21
F%+ - - - - -	3	0	5	8	72	-3	4.96
Mr - - - - -	7	0	4	11	99	+20	5.20
Total Movement++ - - -	7	0	4	11	99	+41	5.41
No Shock - - - - -	2	0	1	3	27	+10	5.37
CF+ - - - - -	3	1	1	5	45	+3	5.07
C+ - - - - -	0	0	0	0	—	—	—

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A PARANORMAL DREAM?

FOR this report we are indebted to the Rev. Canon L. W. Grensted, until recently Professor of the Philosophy of the Christian Religion in the University of Oxford, and a member of the Society of long standing.

* * *

The dream which follows, though of uncertain evidential value, is of considerable interest, since it is difficult to regard it as simple coincidence. It was brought to me by a friend in the course of an analysis, based upon a series of dreams, and I have sent his name and original manuscript to the Research Officer of the S.P.R. to be placed on record.

The dream, which falls into two related parts, was written down by the dreamer immediately on waking on the morning of Monday, August 28th. He had not then seen the morning paper, nor had he heard the B.B.C. news at any time on the previous day, his work as a clergyman having indeed kept him more than fully occupied on the Sunday. He had no conscious knowledge of the railway disaster to the through express train from Holyhead, which collided with a light engine at 3 a.m. on the Sunday morning, six people being killed and many injured. He first saw the news in a morning paper, not the *Daily Telegraph* (see below), at breakfast time, and as he brought the dream, with some others, to me at 11 a.m. that morning, the whole record falls short of the more exacting standards of evidence. But he is not the sort of

person whose word need be doubted. The manuscript which he shewed me was one of a series, and I am quite sure that, like those of his other dreams, it was an entirely innocent original. We were discussing his dreams with a definite purpose far removed from psychic matters. He told the dream without any signs of special interest in it, or indeed of understanding it at all, and I am convinced that he was quite genuinely surprised when I remarked upon the coincidence between his dream and the accident in North Wales.

His record of the dream is as follows :

(a) I was going for a walk and came to a railway. There was a breakdown crane on the track and three girls who were ahead of me climbed up onto the crane, and asked the man in charge of it to take a photograph of them.

(b) I was in a room overlooking a railway and saw a man writhing in agony on the track. I got the impression that his spine was broken. The driver of a breakdown crane came and tried to lift him up, but failed. He then put the hook of his crane through his belt and raised him so that he was crouching on his hands and knees. He then went to get help.

At the time when his dream was read to me neither the dreamer nor I myself had seen the *Daily Telegraph*. It was therefore almost startling to find the following passage in the account of the accident given by that paper :

Nurse Alice Mulrannen said : The first man I found was the driver of the other engine. He had spinal injuries and shock. I gave first aid and stayed with him until a doctor arrived to give him morphia.

This final coincidence is much more striking than the rather general parallel to the accident which the dream had presented at a first hearing. For even if the dreamer had in fact heard, without consciously noticing it, some reference to the accident during the Sunday, it is extremely improbable that such a reference would have included this specific detail about a particular man with a spinal injury. Nor would the B.B.C. announcements, even if he had heard them, have done so.

But quite apart from this point, the dream at once attracted my attention by its special character and imagery. I had the advantage of comparing it with a previous series of dreams and dream fragments, and there had been nothing whatever to suggest any hint of sudden death, or the violence of a railway accident. Nor had trains or break-down cranes played any part in the analysis at all. The dreams had concerned themselves with two other main psychological motifs, and the one link with the earlier dreams is to be found in the appearance of the three girls, who

have nothing to do at all with the possible paranormal elements involved, but who have, symbolically, a very great deal to do with the problems with which we were concerned. With that clue it was easily possible to give the whole dream, with its break-down crane and its very unorthodox method of rendering first aid, a significance plain enough in its place in the analysis. But the source of the symbolism is a different matter, quite distinct from the use which the psyche of the dreamer makes of it, and it is therein that the interest of this dream lies.

The dream is, of course, very unsatisfactory as evidence, though with my own personal knowledge of the dreamer and of the whole series of his dreams I find it difficult to avoid the conclusion that some form of paranormal awareness was involved. What is perhaps of more importance, and, indeed, a main reason for recording the dream at all, is the indication which it offers of evidence specially accessible to psycho-analysts, who are in a position to study dream-material upon a broad basis of knowledge about the dreamers concerned. The possibility of telepathic influence (using the term in its broad, popular sense) upon the symbolism of dreams has been suggested more than once, and, indeed, both Freud and Jung have recognized that dreams may sometimes reveal an extraneous 'psychic' influence. It is highly desirable that even uncertain examples should be recorded, both for the sake of comparative study and in order to give the evidence its full cumulative value.

* * *

In reply to the Editor, Canon Grensted stated that, so far as he knew, he himself had no knowledge of the railway accident until he saw the newspaper on Monday at about 8.45 a.m., i.e. after the dream took place and was recorded. He might, he said, have heard somebody in his family speak of it after the 9 p.m. wireless news on the Sunday, to which he himself did not listen. He had certainly heard or seen no mention of spinal injuries. He does not take the *Daily Telegraph*.

Both Canon Grensted and his friend were in Oxford throughout the week-end.

OBITUARY: MRS LEONORA PIPER

THE death of Mrs Piper on 3 July 1950 at the age of ninety-one brought to a close an important chapter in the history of psychical research. Her phenomena, remarkable in themselves, and far more continuously studied by competent observers than had been

possible in earlier cases, immensely strengthened the evidence for the existence of paranormal faculties in trance-mediumship. In his Introduction to the first report on Mrs Piper's phenomena (*Proc. S.P.R.*, VI) Myers says that 'the study of trance utterances is, indeed, at first sight distasteful'. It is in some measure due to the striking nature of her phenomena, and to the personal integrity of Mrs Piper's character, that no one would now think such an apologetic opening necessary.

Mrs Piper's mediumship began in 1884. Its immediate inception resulted from her consulting for reasons of health a 'psychic healer' named Cocke; at her second sitting with him she herself went unexpectedly into trance. That her mediumship in its early days was partly shaped by this connexion with Cocke is shown by the fact that one of her early Controls was Bach, who had also been one of Cocke's Controls, and Dr Phinuit, who described himself as a French doctor and was one of the most important and persistent Piper Controls at this stage, had an obvious connexion with Cocke's Control Dr Finney, also said to be a French doctor.

In 1885, almost at the beginning of her mediumship, Mrs Piper's phenomena were brought to the notice of William James, who had several sittings with her and became convinced of her paranormal powers. Two years later, in 1887, Richard Hodgson went to Boston to take charge of what was then the American Branch of the S.P.R., and from that time until his death in 1905 he was mainly responsible for all sittings held with Mrs Piper in America. In 1889, in consequence of favourable reports received from observers in America, especially James and Hodgson, it was decided to invite Mrs Piper to come to England, and an important series of sittings was held between November 1889 and February 1890. The report on these sittings, the major part of which is by Myers and Lodge, includes a contribution from Dr Walter Leaf (*Proc. S.P.R.*, VI, p. 558) to which I call attention because it is clear from what he says that Leaf was sceptically inclined. But he also rejects as untenable the hypothesis that all knowledge shown in the trance could be derived from normal sources, and asserts his strong conviction of Mrs Piper's honesty.

Her mediumship entered on its second phase in 1892 with the death of the Communicator (and subsequently Control) known as G.P. Personal communications of the type given in the G.P. sittings will always be more convincing to the sitter than they can be to any reader, depending in part on a general impression of personality. Nevertheless, these sittings (reported on by Hodgson in *Proc. S.P.R.*, XIII, pp. 284 ff.) afford some of the most striking evidence of that kind for communication with the

dead. It was during this phase of her mediumship that automatic writing began to replace oral communication in Mrs Piper's trance. During the third phase of her mediumship, which began with the advent of the 'Imperator Band' of Controls in 1897, oral communication ceased (except for a few words spoken in the 'waking stage'), all communication during the trance being made in writing. The Imperator Band of Controls claimed to be the same personalities as the Controls of Stainton Moses, whose identity was not then publicly known. But they were never able to substantiate their claim. Stainton Moses's Control Rector, for instance, claimed to be St Hippolytus, but Mrs Piper's Rector (her chief Control during this period) never made this claim, nor was he ever able to establish his identity with any person who had ever lived on earth.

In 1906, the year after Hodgson's death, the S.P.R. invited Mrs Piper to pay another visit to England, partly in the hope of obtaining her co-operation in the cross-correspondences which had recently been initiated among a group of automatists in this country. As an example of this type of phenomenon may be mentioned her contribution to the 'sevens' cross-correspondence (*Proc. S.P.R.*, XXIV, p. 252).

In 1911 Mrs Piper's trance phenomena came to an end on the initiative of the Controls. The trance was very deep and return to a normal state increasingly difficult. The Controls gave it as their opinion that it would be unwise, not to say dangerous, for Mrs Piper's mediumship to continue on these lines. Their advice was followed and the Control called Imperator 'closed the light'. From this time on Mrs Piper practised automatic writing without trance. The Faunus incident (*Proc. S.P.R.*, XXIX, pp. 111 ff.) belongs to this period.

The fact that a large number of complete records of Piper sittings were available gave an opportunity for a psychological study of her mediumship, and it was fortunate that this task fell into the admirable hands of Mrs Sidgwick. Her report (*Proc. S.P.R.*, XXVIII) constitutes the most important and complete psychological study of trance-mediumship hitherto published. Mrs Sidgwick, while expressing without any doubt the view that the knowledge shown in Mrs Piper's trance communications must often have had a paranormal source, differs from some observers, e.g., Hodgson, in holding that 'the intelligence communicating directly with the sitter through Mrs Piper's organism is Mrs Piper herself', that is to say, she regards the Controls as in the nature of secondary personalities.

About 1910 several of Mrs Piper's sitters, who wished to show their warm appreciation of the help they had received through her

mediumship, collected a fund of which she was to receive the income. The Society was glad to take this opportunity of recognizing her immense services to psychical research by agreeing to supplement the income by an annual grant. This arrangement was at Mrs Piper's request terminated in 1932, when all but a small part of the capital of the fund was transferred to her.

I had a few sittings myself with Mrs Piper during her visit to England in 1906-7. It had been hoped at this time to obtain communications from Hodgson, and I was chosen as one of the sitters because I had known him as a child. These Hodgson sittings, including my own, were from an evidential point of view disappointing, but I am glad I had an opportunity to witness the Piper trance, which was an interesting and impressive phenomenon. It was, as I have said, a very deep trance, far deeper than that of any other medium I have seen. All life seemed to be concentrated in the right hand, which not only wrote the communications, but turned itself about in a dramatic manner to 'listen' to any observations the sitter or note-taker might make.

At the time I saw her Mrs Piper was of a stout, placid, matronly appearance and there was nothing in her manner or looks to suggest abnormality of any kind. She was well liked and respected by all who knew and worked with her, and her relations with her two daughters, Alta and Minerva, were warmly affectionate to the end. The path of the professional medium is not always an easy one to tread, but Mrs Piper seems to have evolved from her experiences a quiet and rather detached philosophy of life. All who are interested in the problems of psychical research have cause to remember her with gratitude.

H. DE G. S.

REVIEWS

INTO THE UNKNOWN. Report of an Investigation into Psychic Force by a Panel of Independent Experts. London, Odhams, 1950. 200 pp. 5 plates. 10s. 6d.

This book is worth reading by anyone who wishes to know what is to be expected at an average sitting with a contemporary medium; average, because, although several of the mediums investigated are well known, one misses the name of Mrs Leonard, through whose mediumship our Society has obtained outstanding results.

It is in several respects an odd book. There is no indication

who chose the panel of eight experts. Three are members of the Society, Lord Amwell, Dr Bendit, and Mr L. A. G. Strong. The other five would probably disdain for themselves the description of expert in psychical research, but as persons of eminence in various ways, and all devoid of hostility to the paranormal, they were good choices for the task they undertook.

Nor are we told who selected the mediums for investigation, but here again little complaint need be made, as apart from the absence of Mrs Leonard the list is fairly representative of most types of mediumship.

The Introduction, the author of which prefers to blush (we hope) unseen, contains little of value apart from a list of members of the panel with brief biographical notes. In a popular exposition too great technical precision is to be avoided, but a passage like the following (p. 21) can only mislead the uninstructed, and baffle comment by a reviewer :

Paranormal cognition is something which the early investigators such as Freud and William McDougall accepted under the more readily understood expression 'the sixth sense'. The fact that paranormal cognition is a term to-day readily accepted among medical psychologists is in itself an admission that men of science admit the presence of the 'extra-sense'.

There follow extracts from the records of the sittings with the different mediums, with comments on each sitting from some members of the panel, six of whom, in the section entitled 'Concluding Essays' give their opinion on the whole series and on mediumship in general. It is significant to note what a very poor impression, as regards evidence for spiritualism, these typical sittings made on all but one (Sir John Anderson, Bart.) of a group none of whom could be accused of materialist bias.

As Mr Hannen Swaffer, in his 'Postscript', while criticising several members of the panel, makes no complaint that the extracts were not representative of the complete records of the sittings, one must assume that they are not unfavourable specimens of contemporary mediumship. But to judge any individual medium fairly, complete records of several sittings with full annotations are necessary, and the scale of the book does not admit of this.

Of Mr Hannen Swaffer's 'Postscript' it would be kinder to say nothing, were it not necessary to correct a glaring misquotation of an S.P.R. publication. He is referring to a passage in Dr Matthews's Myers Memorial Lecture (*Proceedings*, Vol. XLVI, p. 2) in which Dr Matthews remarks that what was perhaps unexpected by many members of the Archbishop's then recent Commission was the evidence that a number of people had found

in psychical research a confirmation of their Christian faith. Mr Swaffer (p. 194) closely paraphrases the sentence, but substitutes 'Spiritualism' for 'psychical research'.

W. H. S.

TELL ME THE NEXT ONE. By John Godley. London, Gollancz, 1950. 130 pp. 6s.

The whole book consists of description and discussion of a series of dreams in which the Hon. John Godley foresaw correctly the winners of a number of horse races. There were seven dreams in all, in the course of which there figured the names of ten horses. Of these horses, eight won first place, one won third place, and only one was unplaced. At first sight this seems something more than a chance coincidence, and on the cover of the book there is printed the provocative heading—'Chance? Odds against: 730,406,249 to 1.'

It is not possible to work out the exact probability of a specified horse actually winning, but a very rough estimate might be obtained from the bookmakers' starting odds in the following way. Suppose Mr Godley had betted two shillings on each of his ten horses, at the end of the transaction, after a total expenditure of £1, he would have received £33 13s. in winnings. Thirty-three to 1 seems a better estimate of the anti-chance odds than Mr Godley's figure, which is arrived at (p. 76) as follows: The first eight names only are selected, and from among these the one that lost is omitted. Since all failures have been excluded, the odds can then be multiplied together. Each set of odds is also multiplied by four, because it is estimated that the odds are four to one against a predicted horse running at all in the two days following a dream. The difficulty about accepting this last argument is that no one can know whether Mr Godley was subconsciously aware what horses were likely to run.

The case loses interest when it appears that chance is a reasonable explanation. Further details can be found in the *Journal* for June-July 1947 (pp. 63-8), where most of the dreams have already been recorded and discussed.

D. J. W.

THE LIFE OF THE WORLD TO COME. By R. J. Campbell, D.D. London, Longmans, Green, 1948. viii, 192 pp. 12s. 6d.

IS GOD EVIDENT? AN ESSAY TOWARDS A NATURAL THEOLOGY. By Gerald Heard. London, Faber, 1950. 252 pp. 12s. 6d.

These two books, both theological studies in the broadest meaning of the term, though widely different alike in their pre-

suppositions and in their conclusions, have it in common that they take psychical research seriously and are not disposed to reject results which neither religion nor science has hitherto found it easy to swallow. Their interest for psychical research is not that they have anything of importance to add as to evidence, methods, or conclusions, but that they suggest possible correlations with other fields of study, and thereby help to break down the unnatural isolation in which, to a large extent, the subject still stands.

Canon Campbell's book is a comprehensive survey of the Christian teaching about immortality, and those who may have assumed that that teaching is simple, straightforward, and, perhaps, out of date, will be surprised to find how complex its history has been and how flexible it is in its main conclusions. His purpose in writing is frankly directed against those who reject the Christian position on Spiritualist grounds, and he maintains his argument not by the flat rejection of all Spiritualist evidence, but by showing how little conflict there need be between the Christian view and passages which he quotes from the writings of Myers and the communications received and made public by Mr Drayton Thomas. It is not important to his discussion to enter into any detailed analysis of the evidential value of such communications, and the interest of his book, very considerable for theologians, will lie largely for students of psychical research in the breadth and vitality with which the Christian position is stated.

Mr Heard, as might perhaps be expected, is more exciting. He is not concerned for orthodoxy, either in religion or in science, and has a good deal to say about the causes and present state of the alleged conflict between the two. His book is that of an adventurer upon the frontiers of knowledge, and of an adventurer unusually well equipped, by wide reading in many fields, for pioneer work of a kind compelling in its interest. He is, of course, painting upon a canvas far larger than that used by Canon Campbell, and with a much more varied palette, though a reading of Canon Campbell's book might perhaps have persuaded him that even Christian orthodoxy is more flexible and more in touch with the realities of the universe than he seems disposed to allow. The great value of his book is that it is an essay in the proper sense, an attempt at understanding and synthesis, taking all the evidence into account. Therewith it challenges the canons and limitations of science and theology alike, but the answer suggested comes out far more on the side of religion and a religious hope for man than on the side of science with its systematic rigidities.

In framing his argument Mr Heard takes the findings of parapsychology fully into account, and he is well informed as to the recent work which is being done. His book is of real value as a

possible picture of the universe as we are now coming to know it, taking into account not only the assured facts but also the margin where those facts shade off into hypotheses, and where the work of Rhine and Soal has to be given its place beside that of the most recent physicists and astronomers. It might well be read side by side with the recent articles by Professor H. H. Price in *Enquiry*, which supply an adequate philosophical approach to this sort of essay. He would be the last person to claim any finality for a survey of this kind, and, indeed, expressly disclaims any such finality. But there is much virtue in a preliminary sketch, especially when made by a writer with Mr Heard's wide interests and inquiring mind.

L. W. GRENSTED

THE FOX SISTERS. By Magdalen King-Hall. London, Peter Davies, 1950. 269 pp. 9s. 6d.

A problem that often arises when a novelist writes about real people—in the historical sense—is that he may be obliged to forfeit his power to make their story interesting and dramatic. His imagination is able to organize the material, to keep the various episodes in proportion, and to ensure a continuous flow of development for the story; but real life may hamper him. Real life frequently neglects so to consider the reader, condemning the characters to long periods of repetition, even of dullness; and if the novelist wishes to be faithful to what happened in fact, he runs the risk of writing a dull and ill-organized novel.

Miss Magdalen King-Hall is too old a hand to let her narrative be dull, but even she has not been able to give the story of the Fox sisters a continuous development and flow. I am at a loss to know how best to approach her book—whether to treat it as a novel, or a history of fact. As a history of fact, it is a little disfigured by certain devices of the novelist, such as insight into the mind of this or that sister, and attempts (with which I heartily sympathise) to give the narrative movement and suspense. As a novel, it suffers from long, flat periods when the same sort of thing went on happening, and from the ludicrous vacillations of Dr Elisha Kane, the explorer, Margaretta's sweetheart. The real Dr Kane is only raw material for the novelist, not a finished product. I imagine, however, that readers of this journal will be more interested in the book as a history than as a novel, and particularly interested in its treatment of the sisters' mediumship. This Miss King-Hall regards as wholly fraudulent, the product of a curious gift for making their toe-joints click. At the same time she speaks, near the beginning of the book, of poltergeist

phenomena, and suggests that genuine disturbances, supposedly connected with a murder, were heard in John Fox's house. I personally felt a slight degree of indecision in Miss King-Hall's treatment of the psychic, or so-called psychic, aspect of the story ; but I hasten to add, as a practising novelist, that the whole subject presents a writer with great technical difficulties, and that the line between a novel about mediums and a treatise on mediumship needs to be drawn firmly, and is extremely hard to draw.

From whatever point of view one looks at it, the whole story of these two attractive and suggestible girls, from their primitive beginnings to the national fame which they enjoyed when at their heyday, is interesting, curious, and sad.

L. A. G. S.

THE PHYSICAL PHENOMENA OF MYSTICISM. By Montague Summers. London, Rider, 1950. 263 pp. 12 plates. 21s.

The title of this book is not inappropriate for its contents : for it has little meaning. The reader of the *Journal*, unlike the man in the street, will guess that by 'physical' in this context the author means something more like 'paranormal'. While this gloss elucidates the author's intentions, it does not help to give meaning to the title of his book : for it is precisely one of the outstanding characteristics of mysticism that it is free from the paranormal (in our sense). Even Mr Summers, whose erudition at least is not in doubt, has sought in vain for 'physical' or any other paranormal phenomena in the lives of the true mystics, Bernard of Clairvaux, Meister Eckhart, Ruysbroeck, François de Sales, Richard Rolle, and their like. By 'mysticism' the author means something quite different, and an accurate title for his book, though admittedly not a very vendipotent one, would be 'The alleged paranormal phenomena associated with religious hysteria'. What are these phenomena according to Mr Summers? He sets them out on page 79, where they can be seen to include stigmatisation, levitation, bilocation, 'supernatural inedia', telekenesis (*sic*), 'infused knowledge', 'supernatural empery over nature', incendium amoris, etc.

Does the author apply critical standards to his material? He does go so far as to quote evidence for the post-hypnotic production of stigmata, and he comments (p. 122), 'The underlying power which causes the results of such experiments . . . is not difficult to discover. It is simply demonianism. The "mechanism" is the fiend.' The reviewer regrets his lack of competence to comment on this no doubt well-founded *ex cathedra* pronouncement.

On page 246 the author states that 'Scientists now honestly confess that they are baffled' by Theresa Neumann. Immediately before these words Mr Summers writes, 'The Bishop of Ratisbon still regulates very strictly any visits to the Neumann household. In fact nobody is admitted without his authorisation, which is far from easy to obtain.' What is one to say of an author, writing on such a subject as this, who can put down these consecutive sentences without any perceptible qualm?

Perhaps only one more thing need be said. Mr Summers writes in the language of a devout Roman Catholicism. Indeed, on the back of his titlepage is a Latin declaration which a careless reader may take for some kind of imprimatur—which it is not. Now it may be doubted whether anyone has written on this subject and from that point of view more authoritatively and more voluminously than the late Herbert Thurston, S.J. Yet Father Thurston's name will be sought in vain in these pages.

TH. B.

ALL AND EVERYTHING. By G. Gurdjieff. London, Routledge and Kegan Paul, 1950. ix, 1238 pp. 30s.

IN SEARCH OF THE MIRACULOUS. By P. D. Ouspensky. London, Routledge and Kegan Paul, 1950. xi, 399 pp. 30s.

A recent number of this journal contained a plea to psychical researchers occasionally to risk the contemplation of a speculative hypothesis. Any reader of these two books must be prepared for a moment to let his mind wander thus far beyond the bounds of strict orthodoxy, if he is to get any value from them.

The author of *All and Everything*, Gurdjieff, who died last year, was an enigmatic figure of Russo-Greco-Armenian origin. He was remarkable for his effect on people of more than average intelligence and for his hypnotic and other apparently unusual powers, and he claimed to transmit orally, to chosen pupils, authoritative teachings about the nature of the Universe and of Man, which he said he had received from the Hidden Brotherhood of occult tradition. This teaching he proposed to embody in three books, and the first of these, *All and Everything*, is intended to clear the way by 'mercilessly destroying' all its readers' current views and beliefs. The book is a long, involved, and fantastic allegorical satire—the satire is painfully apt—which leads to the conclusion that nothing man can do to his exterior environment can improve his present miserable condition. His only hope is to learn the exceedingly difficult task of changing himself.

This oft-repeated warning would probably have been totally ignored, as usual, by the erudite, had it not been for the out-

standing personality of Gurdjieff's collaborator, P. D. Ouspensky. For Ouspensky is not easy altogether to ignore. He first met Gurdjieff in Moscow in 1915, after many years wandering in search of the miraculous. Of this he writes: 'I already knew . . . that beyond the thin film of false reality there existed another reality from which, for some reason, something separated us. The "miraculous" was a penetration into this unknown reality' (page 3). Gurdjieff he found to be ahead of him along this path of penetration, and he also found that Gurdjieff's teaching corresponded to an astonishing degree with his own conclusions, already recorded in *Tertium Organum* and *A New Model of the Universe*. The two men worked together for many years, and the book, *In Search of the Miraculous*, is an account of that work and of Gurdjieff's oral teaching. Whatever our view of the theories it contains, few could remain unimpressed by the sweep, lucidity, and coherence of their exposition. Although a book for the philosopher, the psychologist, and the physicist, it is written with such simplicity that many laymen will find it as hard to lay down as any story of physical adventure. Its cosmology has been said to be 'uncannily consistent with the latest theory of creation' of astro-physics.

Ouspensky postulates the division of the universe into seven 'cosmoses', and much of the book is concerned with their relation to one another, their physics, and the nature and number of their dimensions and respective 'times'. But it is his theory of the nature of man which is of chief interest to psychical researchers. Man is, he says, a microcosm of the universe, divisible also into seven levels. (For Ouspensky the law of octaves is the basic law of life, and every entity, organic or inorganic, has its own intelligence.) But man, as we know him, functions only on the lower three of these seven, through three 'brain-centres', the instinctive-moving, the emotional, and the intellectual. He falls into three types, which Ouspensky labels Man Nos. 1, 2 and 3, according to which centre is most in evidence. But with only these three centres functioning, which is man's 'normal' condition, he is in reality 'asleep'. And he is, in effect, a mere machine, impelled to action only by what 'happens' to him from without. Not until he becomes Man No. 4 can he be genuinely conscious and capable of objective vision. The consciousness of Man Nos. 5, 6, and 7 is progressively greater.

Traditionally, the attempts of Man Nos. 1, 2, and 3 to become Man No. 4 have been made by way of the Fakir, the Monk, or the Yogi, respectively. But these ways entail one-sided development. Ouspensky teaches a fourth and yet more arduous way, which seeks to develop all three lower centres concurrently.

Hence the training must be carried out while still living *in* the world, although periods of silence, contemplation, and 'self-remembering' are essential. The ultimate aim is to bring man's consciousness into relationship with the whole universe. To Ouspensky all further evolution means the evolution of consciousness, though he insists that each level of consciousness functions through its own vehicle, which interpenetrates all the others. Nothing, he says, can manifest without a vehicle.

Ouspensky offers no proofs of his teaching about ultra-physical states of being, because he does not consider it possible for anyone to understand a condition or a dimension he had not himself experienced. The work *must* be done to learn the doctrine. He records a number of psychical experiences of his own—some apparently veridical—and his accounts resemble those of other psychics. He also gives a few hints and clues about the fourth way of becoming Man. No. 4, though he insists that this work cannot be done without a personal teacher. In Chapter 13 he speaks of various exercises and experiments upon himself and his consequent change of consciousness. But of this he says: 'I now come to a most difficult thing, because there is no possibility whatever of describing the facts themselves. . . . I remember that assertions of this kind always made me indignant when I came across them in the memoirs or notes of people who had passed through any sort of extraordinary experiences. . . . They had sought the miraculous. . . . But when they had found what they sought they invariably said: "I cannot describe what I have found." It always seemed to me to be artificial and invented. And now I found myself in exactly the same position. . . . I saw and observed facts that entirely transcended the sphere of what we consider possible, acknowledged, or admissible, and I can say nothing about them' (page 261). It is the same old story. The mountain will never come to Mahomet. He can produce evidence that it exists from where he stands, but he must go to it to learn more about its real nature.

It would be easy—and orthodox—to write off this book as fantastic speculation, though it would be difficult to deny the quality of a mind which can expound such a cosmology and psychology coherently and intelligibly in 400 pages. But it would be unwise to ignore its penetrating diagnosis of our ills and shortcomings. [And it might even be interesting for the adventurous to classify its hypothesis into 'untenable in the light of known facts' and 'not, although unproven, incompatible with any known facts.'

R. I. H.

NEW FRONTIERS OF THE MIND. By J. B. Rhine. Harmondsworth, Penguin Books, 1950. 1s. 6d.

Four years ago Penguin Books published Mr G. N. M. Tyrrell's *The Personality of Man*. By issuing Dr Rhine's *New Frontiers of the Mind* they have made another valuable contribution to the task of spreading knowledge of research in parapsychology among the general public.

New Frontiers of the Mind, as members of the Society will know, was first published in 1937, and gave an account of the first seven years of work on E.S.P. at Duke University. In the present edition, Dr Rhine has included a supplementary chapter briefly reviewing experimental work on both E.S.P. and PK in America and Britain down to the end of 1947. The book is essential reading for all students of psychical research.

E. O.

LES RADIESTHESISTES DEVANT L'OPINION PUBLIQUE. By G. Jacquemyns. Brussels, INSOC, 1950. 55 pp.

This bulletin is issued under the auspices of the Belgian Institut Universitaire d'Information Sociale et Economique (INSOC), a body which studies the state of public opinion with a sampling technique. Its inquiry into certain divining phenomena was suggested by the Comité Belge pour l'Investigation Scientifique des Phénomènes réputés Paranormaux, a body formed towards the end of 1948.

The inquiry examined the extent of public belief in divining of various kinds, and found the proportion of the population who had consulted diviners. The results of these consultations were also compiled and analysed. Inquiries were made into five different claims made by diviners: to find water, to determine the sex of an unborn child, to discover whether missing people are dead or alive, to trace missing people, and to diagnose illness.

Nearly half the people questioned believed that certain people can divine water, and about a quarter believed in the medical diagnosis, whereas only 10-15 per cent accepted the other claims. Only 6.3 per cent, however, had actually used the services of diviners in water finding, of whom 85 per cent reported the divining to be successful. The success in determining the sex of an unborn child was 52.5 per cent, according to the 2.8 per cent of people who had tried this method. It is only slightly greater than the 50 per cent success which would have been obtained by guessing. The success obtained by diviners in finding whether missing people are living or dead is reported as 58 per cent, and in tracing missing people as 42 per cent. The success in medical

diagnosis was 36 per cent. These figures, and many more, are analysed at length in the bulletin.

It is clear that in Belgium many people have had recourse to diviners for consultations of various kinds. The analysis of the results they report throws little light on the question of the reality of the claims made by the diviners. Often the diviners made vague statements. For example, many of the consultations concerning missing people were made during the war, and the diviner merely indicated the person to be towards the East. One person who visited six diviners to inquire whether his son was alive obtained two opinions that he was, three that he was not, and one formula sufficiently vague to be interpreted in either sense. Thus the statistical analyses alone may not reveal many important considerations. Whereas those who answered 'No' when asked whether the statements of diviners were subsequently verified usually gave a categorical answer, those who answered 'Yes' frequently added qualifications such as '*n'était pas fort loin de la vérité*' or '*était une indication d'une certaine utilité*'. The bulletin reaches no conclusions concerning the reality of the claims of diviners.

A. J. B. ROBERTSON

CORRESPONDENCE

THE LAWS OF EVIDENCE

SIR,—I am sorry Dr Thouless is so cross. I am even sorrier that he considers me guilty of a fabrication. And I am a little surprised at being accused of being an armchair critic.

May I suggest that the main body of Dr Thouless's letter is irrelevant? If he did not say what I said he said my previous letter was pointless and there was no need to answer it. The matter is easily settled. Several other members of the society were present at Brighton: if they will come forward to endorse Dr Thouless's recollection of what he said I will gladly withdraw my letter, apologise to Dr Thouless, and do penance.

THEODORE BESTERMAN